The Logic of Comparative Theory Evaluation

The paper is divided into six sections

1. Critique of Schaffner's account
2. Bayesian analysis of Ad hocness
3. The case of multiple predictions.
4. Quantitative predictions
5. Bayesian Model for Methodology of Research Programmes
6. An alternative approach - The Methodological Decision.

Origin of the investigation

Schaffner's critique of Zahar's analysis of the Lorentz & Einstein research programmes in which he presents a Bayesian ~~model~~ application for ad hoc$_2$.

Schaffner's mistake In formulating Bayes's theorem he writes

$$P(T/h \& e) = \frac{P(T/h) \cdot P(e/T \& h)}{P(e/h)}$$

and identifies $P(e/h)$ with "the probability of obtaining e on the basis of *theories other than* T" (Schaffner's italics) thus he makes $P(e/h)$ independent of $P(T/h)$ and ends up with a criterion for ad hoc_2 viz $P(T/h)$ should be small in virtue of its lack of "theoretical 'support'"! __But__ this looks like ad hoc_3 !

Before giving a corrected analysis I make some __general remarks__ on the notion prob (theory is true / background information) ($P(T/h)$)

(1) We distinguish the statement

__A__ "T is true"

from the statement

__B__ "In the light of present evidence I possess a degree of rational belief $P(T/h)$ that ~~the theory~~ T is true"

__A__ is connected with the notion of __verisimilitude__

T is "surveyed" by God for all its true and false consequences and a measure of "__nearness to truth__" is imposed -

We are adopting here a __realist__ approach to theories and not necessarily confining ourselves to true & false __empirical__ content.

Practical difficulties in defining such a measure cf Tichy - Miller criticism of Popper's original proposal for ~~the~~ ordering the verisimilitude of false theories .

Query. If two theories have identical "empirical" verisimilitude how can ever arrive at a comparison of their "metaphysical" verisimilitude

Reply : (1) To a realist metaphysical verisimilitude 'exists' whether we can know it (by intuition ?) or not.

(2) One may believe that there is a unique *simple* theory that will give *all* correct empirical predictions

Notes Discoveries in *theoretical* physics appear to proceed by some sort of intuition of *natural orders of classification* (Duhem) or by *adaptation* of mind to matter via direct "confrontation" (Blackwell)

Realist view of theories alone accounts for *success* of theories (cf Kant's question How is science possible — Prolegomena.)

Instrumentalist view cannot account for *predictive* successes (only confirms what is already known)

* Commitment to a theory means not that one believes it to be true but that one believes (1) For current domain of experimentation it gives same observational predictions as true theory ~~(2)~~ i.e. belongs to $\{T_i\}_e$.

(2) True theory "generalizes" in some sense theory being considered i.e. satisfies general Correspondence Principle

(3) Given theory is superior to competing alternatives in respect of (1) & (2)

Schimony, Scientific Inference, 1970.

(B) investigates the question "To what extent do we believe our theories to be true, and how rationally should we modify this belief in the light of predictive success

Various possible explications of $P(T/e)$

(1) Subjective coherent betting quotients

– possibly not applicable to scientific theories – non-decidability of bets, also involves ~~[illegible]~~ notion of utility or practical effectiveness of theories which seems again close to instrumentalism

(2) Subjective ~~probabil~~ degree of belief subject to intuitively-known rationality constraints (satisfaction of probability axioms)

(3) Degrees of commitment rather than belief ~~(scientists commit themselves to one theory at a time~~ cf Shimony's tempered personalism *

But commitment is measured by actions and in turn measures belief

actions → commitment → belief.

~~[illegible]~~ three main questions to settle:-

① Do scientists really give non-vanishing probabilities to universal laws.

Answer: (1) they could do if different laws were ordered appropriately cf Jeffreys, Wrinch.

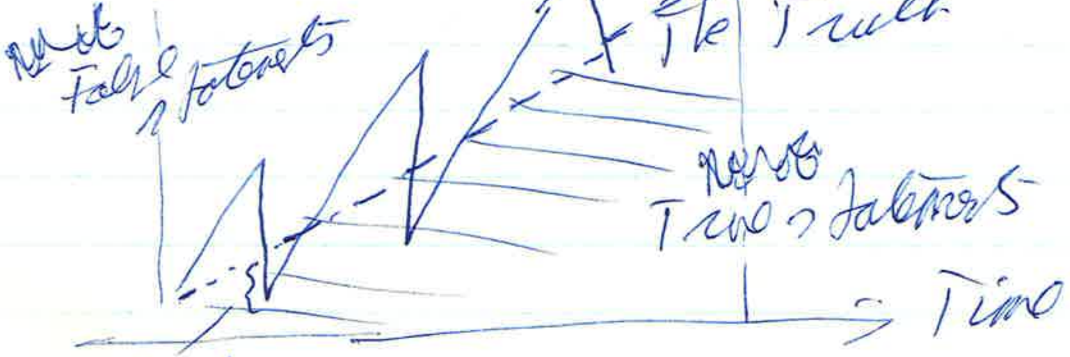
(2). Scientists only believe in truth of theories over a restricted class of phenomena i.e. where "other effects" do not intervene, cf Ceteris Paribus clause.

e.g. at high energies, or small distances, or low temperatures new effect may arise. class of phenomena is usually defined in terms of ranges of certain parameters which characterize the conditions under which a theory is expected to apply.

cf low energy nuclear Physics, applies to all nuclei
or non-relativistic QM applies to all molecular & atomic problems (assumption is - complexity does not produce "emergent" features that could be resolved in terms of two body interactions).

* one might suppose that as science progresses the number of theories in the equivalence class $\{T_i\}_e$, dwindles to a unique "true ~~theory~~" with increasing evidence" e.

II Zig-Zag model for theory growth:


more
False statements
The Truth
more
True statements
Time
kuhn losses

② What happens if two theories T_1 & T_2 give identical predictions for a class of phenomena identified by certain ranges of parameters as discussed under ①?

In this case our belief in the two theories will depend on background knowledge? (e.g. this will include prior appraisal in terms of degree of simplicity)

If we cannot distinguish theories by any background knowledge then we must introduce an equivalence class of theories which all explain e and are equal in simplicity.

$P(T/e)$ is then probability that T belongs to this equivalence class.

We must await further evidence to distinguish theories placed in the same equivalence class by e.*

Post in general T_1 & T_2 will always be in a non-trivial relation of correspondence, even if T_2 supersedes T_1 for new prediction e'.

So even if T_1 is refuted by e' there will be "lower" theoretical structure which is True (or verisimilitude) at any rate under an appropriate translation of Post's General Correspondence Principle. II

10. If according to Post if a theory $T \in \{T_i\}_e$ then some of its theoretical structure of T will be true or survive even in the unique (?) limit $\lim_{e\to\Omega}\{T_i\}_e = \{\mathcal{T}\}$
Ω is total evidence & $\mathcal{T}$ is The Truth.

③ Objection due to Lakatos says all theories are born refuted i.e. research programmes flourish in an ocean of anomalies.

But if a theory is false, it is false and therefore how can we say we believe it to be true - ?

Ans Anomalies are always viewed provisionally as due to additional factors or causes, not yet allowed for.
They are not regarded by scientists as genuine refutations of a theory believed to be true for a class of phenomena which includes the aberrant anomaly.

In conclusion of this introduction I make some general remarks on the appraisal of theories

Theories can be appraised along a number of different dimensions of appraisal

e.g. Good theories certainly have
1.) large empirical content

2.) large truth content (empirical)

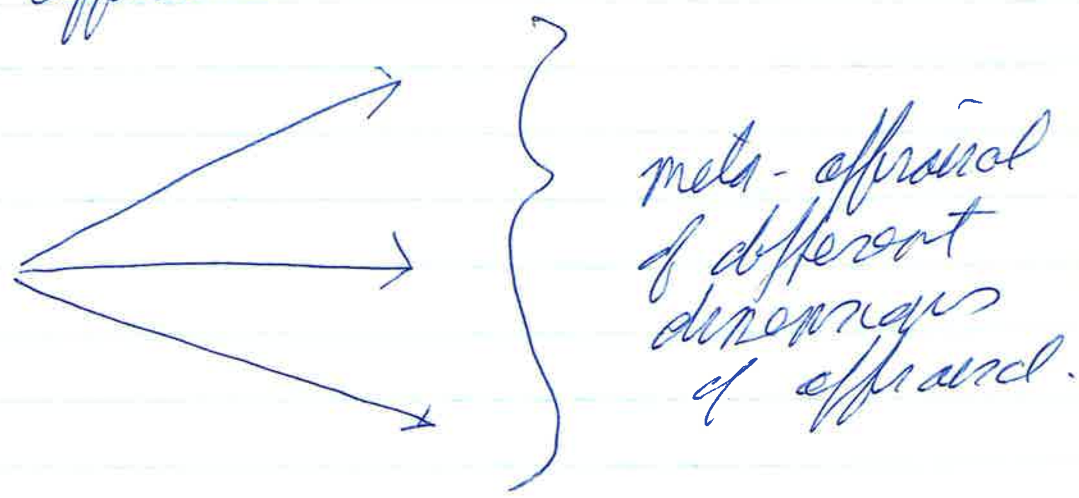
3.) express single unifying idea
i.e. are simple

1) means theories are highly falsifiable
2.) means theories comprehend a large no of empirical facts.
3) means theories are simple

2) leads to Post's criterion $\Delta E > 0$ for improvement of a theory.

the Post criterion goes along well on instrumentalist views of theories.
To a realist we want to know if our theories are true - the dimension of appraisal which tests this aspect of a theory is successful novel prediction i.e. the non ad hoc$_2$ quality.

we are really required to make a multi-appraisal of the different dimensions of appraisal


meta-appraisal
of different
dimensions
of appraisal.

this meta appraisal will depend on one's view of what theories actually purport to do (or be) in science.

I do not dispute DE>0 is one (sufficient) condition for a better theory but I will not regard this as a big theoretical advance if my meta-appraisal is weighted in terms of the "approach to truth" of my theory.

Note on Bayesian Conditionalization

Hacking, Mellor & Hesse support view that Dutch Book argument does not apply to assignment of probabilities in a new decision situation.

But Paul Teller, William Harper & Jon Dorling have produced Dutch Book arguments in support of Bayesian conditionalization.

If we accept the latter's views we can still ask the question (as in section 6 of the paper)

Can ~~methodological~~ methodological decisions be represented as a Bayesian decision strategy

In view of the fact (cf. Hesse) that the constraint of ~~Bay~~ being a Bayesian strategy is a very weak one. I feel answer is probably yes this is open question for experts in statistical decision theory (Prof. Jeffreys) and is now being considered by Teller and Dorling.